

The prospects for a production management body of knowledge in business schools: response to Koskela (2017) 'Why is management research irrelevant?'

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Abstract

This article is a response to Lauri Koskela's recent piece in Construction Management and Economics ('Why is management research irrelevant?' 35(1-2): 4-23) which reflects on the relationship between academic research and management practice in business schools. In particular, Koskela asks why production management research and teaching has disappeared from the business school agenda and why management research has failed to produce a consistent body of knowledge that is of use to management practice. In this article, I try to provide some alternative perspectives on the present and past contexts of management theory and production research. I argue that production research, if not teaching, is alive and well and the site of theory generation, problem-focused research and innovation. I also question the veracity and wisdom of creating a 'body of knowledge' in relation to management research and practice; even if it were possible, which I believe it is not. My assessment of the state of research in business schools, at least in the UK and the US

and notwithstanding a lack of consensus over how to approach management research, is that it is eclectic and vibrant and of much more use to practicing managers in that state.

Introduction

Koskela's work adds to a stream of debate on the rigor and relevance in management research (Bennis and O'Toole, 2005; Mintzberg, 2004; Mintzberg, Simons and Basu, 2002; Ghoshal, 2005; Davis, 2006). Koskela's concern is with the development of a consistent body of management knowledge. Like many before him he perceives a drift toward theory and away from practice of management research. Two key US reports, by Gordon and Howell (1959) and Pierson (1959), Koskela argues, originally set management research in this wrong direction. The report's authors were concerned primarily with the standing and prestige of business schools in the US. They regarded businesses schools at the time as unfocused and second rate – they attracted poor students, non-academic staff and did not advance theory. The result was that business schools lacked the respect of more established theory-driven fields and so did not address broader business and related social and economic questions. A curriculum overhaul was needed. Gordon-Howell (1959), in particular, favoured a focus on strategy, the arts and sciences. Two things resulted from this, Koskela argues. The first was a drift toward theory, primarily toward opaque and introspective quantitative theory and away, therefore, from practice-facing, problem-driven research. The second issue was that production, the key activity that underpins value creation in firms, was dropped as a distinct research and teaching pillar within business schools. The ultimate consequence of this, argues Koskela, is that management research has failed to develop a rigorous, stable,

problem-driven body of knowledge. His article concludes by outlining some emerging areas of research that could form the basis of theory building around the needs of practice.

While I agree that knowledge-building in management research should reflect and support practice, I think it is possible to offer an alternative analysis of the situation past and present – one that offers a more positive reading of the situation. The issue of production's disappearance from the curriculum and as a valid topic for serious research is an important one, but I don't think that we should lay the blame for its demise on a small number of policy reports but, rather, on the post-war economic boom. Moreover, now that Western economies have slowed and anxieties around international competition have established themselves in the minds of policy makers, we should have cause to expect that production research will make a come-back and there is evidence that this has been happening.

I also question whether the idea that a consistent body of knowledge, however practice-focused, is actually a desirable end. I argue that the alternative, an eclectic mix of combative research communities, is just as valid a model to aim for. There is also the question as to whether a consistent body of knowledge is possible in the context of complex, emergent organizations and the realities of management practice. I deal with these issues in reverse order. First, I address the idea of whether a consistent body of knowledge is a desirable state of affairs; second, whether a consistent body of knowledge is possible in the context of management research; third I discuss the proposition that a production focus in research died a natural death in the economic growth of the post war boom, but that it is now in the process of being resurrected.

In a consistent body of knowledge desirable?

Koskela is by no means the first to bemoan a lack of consistent knowledge-building in business schools. Pfeffer (1993) called for the same in organization studies. Pfeffer's widely cited paper is perhaps most interesting for the political advantages he outlines to the creation of such a paradigm. His work identifies that fields with a consistent and durable theoretic paradigm are able to recruit more post-doctorates, gain more resources (internal support and external funding), more efficiently and fairly distribute those resources and make pay and promotion decisions that are regarded as fairer by faculty. Such subject areas also carry more political clout within their universities. Pfeffer's concern is with the prestige of business schools, a different concern from Koskela's, but any call for a consistent body of knowledge is also a call for a consensus, a knowledge paradigm.

Paradigms carry with them certain risks. Knowledge paradigms, while productive, efficient and convincing (to policy makers, VCs and research funders) are also stiflingly conservative (Kuhn, 1962 (1970); Masterman, 1970). Knowledge paradigms are defined by consensus and consensus demands compliance to certain norms, accepted truths and ontologies. High-consensus fields, Pfeffer notes, give rise to strong personal social ties which correlate well with journal publications, grants and membership of journal editorial boards. The long-run tendency, in other words, is with a narrowing mind-set and the emergence of a tightly policed system of knowledge production. The results of this narrowing of mind-set are clear to see in the failure of neo-classical economists to predict the economic crash that beset western economies in 2008. The neo-classical and 'market' orthodoxy that defined the

dominant economics theory paradigm created a community of practitioners that was unable, or unwilling, to see truths beyond those constructed within its own paradigmatic borders and so allowed the crash of 2008 to approach undetected.

This is a normal aspect of knowledge paradigms. The same failing was apparent amongst the aero-engineers who build the Concorde – Britain's first commercial jet airliner. Unable to see outside their existing piston-engine paradigm, they failed to grasp the stresses this new breed of aircraft would have to endure and three were lost in catastrophic airframe failures (Constant, 1980). Schumpeter made that same point when he talked about creative winds of destruction; firms whose thinking is trapped inside the outgoing technology paradigm, fail to see the threat posed by the next technology paradigm (Leonard-Barton, 1993).

Management as an area of research, while it does not form a consistent community of practice, does appear comfortable playing host to a wide variety of perspectives and methodologies. It contains a variety of research communities that coalesce around a number of demanding and rigorous, but very different, journals and conference tracks. These fora, for the most part, serve as vehicle for esoteric theory-building and the practice-orientated application of that knowledge. This seems to me to be a robust and sensible place to be; a mixed and healthy ecology.

It is also worth noting, perhaps, that business schools themselves have not suffered from the lack of a consistent theory paradigm. It has not prevented them being a great success in terms of growth, research output and reputation. One in five of all arts, humanities and social science undergraduates in the UK study in business schools and a little under a third

of post-graduates (HEFCE data 2011). Business schools earn around £2 billion annually for the UK economy. Individually, school's contribution to their regional economies can range up to around the £100 million mark through spin-outs, improved skills and raised productivity in local businesses (Cooke and Galt, 2010).

Is a consistent body of knowledge about management possible?

A consistent body of knowledge may in any case not be applicable to the subject matter in hand. Management is not engineering or medicine – it is a very different, more uncertain practice. Engineers can reliably test the strength of materials prior to assembly, while medics can set-up control groups and lengthy tests before introducing a new drug. In management, experiments have limited reliability – in as much as any test will itself change the context in which management takes place. In other words, a reliable 'management body of knowledge' will always be thwarted, because management only exists as an observable phenomenon 'in action' (Suchman, 1987).

Management practice and the organization that is the subject of that management, are also inseparable and mutually constituting (c.f. Olikowski and Scott, 2015). Suchman argues, for example, that management planning is always achieved in response to contexts 'as they emerge' – what she calls 'situated action'. Action itself is bound up in the shifting

organizational context and "... actors use the resources that a particular occasion provides - including, but crucially not reducible to, formulations such as plans - to construct their action's developing purpose and intelligibility" (p.3).

As Koskela himself observes, quoting Davis (2006), organisations are rather more difficult to deal with than the subject of medical research, human bodies, in terms of building reliable generalisations: "Like a cadaver that keeps jumping up from the autopsy table, the empirical generalizations derived from the study of organizations often get away from us as time moves on" (p.703). But this point, I would argue, is not a critique of a lack of workable theory, but an acceptance that a consistent and durable theory of the organizational body, is simply not possible. Organizations are too complex and varied to capture in a set of reliable, repeatable assumptions. Organisations themselves are emergent restless constructs always 'becoming' rather than 'being': "... a pattern that is constituted, shaped, emerging from change" (Tsoukas and Chia, 2002, p.567). In other words, the cadaver is constantly up and about. Managers, therefore, are best served by a variety of cognitive and material resources to deal with the realities in which they are entangled and, therefore, themselves also constituting – not a limited set of potentially unfalsifiable paradigmatic assumptions.

The rigor and relevance problem in management theory

Koskela's concern with the relationship between management research and management practice reflects an ongoing concern in academe, but it is one certainly not limited to

management research. In the proceedings of the 2005 Architecture Research Futures Workshop it was noted that “The role of research in underpinning the professional and disciplinary knowledge base has not been as much focused as it could have been to date and thus needs more proactive development by academics, professional organizations and practices” (Jenkins, 2005). Science has long-since sought to show the applicability of its work to ‘real life problems’ in order to secure public and policy support.

Koskela, like many before him, poses the idea that theory, in particular numerical social science theory, constitutes a distinct space or pole to which academics can ‘drift’ leading them, and their research fields, away from problem-driven knowledge. This may well be true on an individual level, but it does not necessarily hold when it is recognized that knowledge production in academia is a community effort. At some point academics, however insular, must connect with journals and funders – and neither of these institutions are interested in research that is devoid of any relationship with reality. Academics exist in networks that also, at some point, connect with real word problems. The fact that some academics may prefer to work ‘back of shop’ on the nuances of social-science theory building, does not mean that other academics cannot draw on that work, translate it and apply it. We need to see knowledge production in management research in terms of it being a whole system of interlinked activities, flows and translations. Not every academic is required to be on the practice ‘front line’ of knowledge co-production for the system as a whole to work effectively. Business schools produce different ‘types’ of knowledge from the purely theoretical to ‘Mode 2’ knowledge (Gibbons et al, 1993), co-produced practice-facing knowledge. The question should be re-framed, not as one of which is better suited to

building problem-driven research, but how best to ensure that theory building (in all its guises) and management problem solving, are co-productive of one another.

Previously, I and colleagues, building on Starkey and Tiratsoo's (2006) proposed four categories of business school, based on their differing approach to knowledge production and consumption, developed four types of knowledge generation that can be used to capture the variety of knowledge practices taking place in all business schools (Ivory, et al. 2006). The social science model, defined by its contribution to academic knowledge and debate as measured by the UK Research Excellence Framework (REF); the liberal arts model, orientated towards the fundamentals of knowledge, self-knowledge, wisdom, leadership and art and to the practice and application of these; the professional school, focusing on training; and knowledge economy work, concerned knowledge production defined by an engagement focus and commercial value and produced in conjunction with other organizations.

One of the insights from this work is that the most successful schools, those toping the rankings and generating the most research income, anchored themselves in a social-science model (theory building) but that they also translated that knowledge into knowledge that could be delivered as executive MBA teaching and then further refined through consultancy interactions (Ivory et al. 2007). That is to say, the most successful schools saw no distinction between practice and theory – or at least viewed them as mutually constituting. Moreover, management academics achieve these co-productive interactions without needing to be part of a stable 'body of knowledge'; like Suchman's managers they 'use the resources that a particular occasion provides - including, but crucially not reducible to, formulations such

as plans - to construct their action's developing purpose and intelligibility'. One can simply replace 'plans' with 'theories', 'cases' and 'insights'. Good management academics are not moribund in constraining theory paradigms, but experts in bricolage.

The issue to focus on, therefore, is how best to promote the links between theory and practice in business schools. Academics who are strong 'reflective practitioners' (Schön, 1988) are already good at this. Indeed, this 'linking-work' is also something which both funding bodies and national research exercises (such as the UK's Research Excellence Framework) increasingly demand in the form of evidence that research will, or has had, impact on practice.

The specific nature of the research tools employed should not concern us either. If we start with the proposition that reality is only given up through symbols, language and interactions (Berger and Luckman, 1966) then opaque social-science methods are just another means of accessing, or constructing, that reality. The tools used may be impenetrably opaque and obtuse, but their application is always just another a means of accessing and re-representing reality in some way. We should careful, as Latour (2004) has conceded after some decades of unpacking the mechanics of scientific knowledge production, not to leave ourselves in a situation where conspiracy theory carries as much weight as rigorous research. I would also argue that this was largely the same concern of Gordon-Howell (1959) – that teaching in US business schools was being left to untrained 'quacks' (Economist, 2009). They wanted academics to maintain a sceptical distance from practice, to research practice as objectively as possible, not simply be led by its interests or coloured by its truth claims. Let's not forget that 19th century mill owners argued that child-labour was essential

for them to remain competitive. Research sometimes needs to see beyond the immediate perceived needs of its research subjects to help them move forward. Its' worth noting that Gordon-Howell viewed ethics as an important part of management teaching.

Production focus edged out?

Koskela makes a very interesting point about production teaching and research being edged-out of the mainstream management curriculum. Although laying the blame for this firmly with policy reports on business schools at the time, and they no doubt played an important role, their role lay more, I would argue, in articulating what was already befalling production research and teaching. The more likely cause of production's fall from grace lay in its poor fit with broader economic and management problems, as they were seen at the time.

Koskela quotes Gordon and Howell as arguing:

"Production management courses are often the repository of some of the most inappropriate and intellectually stultifying materials to be found in the business curriculum. Not only do many faculty members have little respect for such courses, but students in a number of schools complained" (1959, p. 190).

It is hard to know the degree to which Gordon and Howell's interpretation of their own data was actively biased against production in order to favour the introduction of their own

subject preferences, but this quote can nevertheless also be interpreted as evidencing an already failing field - academics did not want to research production and students did not want to study it. The rot, as Koskela himself notes, had set in some time before:

“We have all felt, with Professor Schumpeter, a sense almost of shame at the incredible banalities of much of the so-called theory of production ... “ Robbins (1932, p. 65)

Arguably, production research in the economic context of the 1950s could simply not answer the questions that were deemed important at the time. A long-run post-war economic boom was occurring on the back of ongoing industrialization, mass production and automation. The crashes that did occur, as have subsequent ones, were caused by misplaced investment, debt and money-supply problems; but not problems with production efficiency. Throughout the 1950s and 1960s western economies were without serious global competition and their system of production had begun to appear, to policy makers at least, unstoppable.

Such was the confidence in continued technological advance at the time, that Simon (1969) stated (again taken from Koskela):

“In the post-industrial society, the central problem is not how to organize to produce efficiently (although this will always remain an important consideration), but how to organize to make decisions – that is, to process information” (Simon, p.46)

Simon's comments reflected a broader concern at the time about how best to manage the outpouring of new technology, so that it was not squandered - so the problem was one of too much production!

Koskela also quotes Galbraith (1958) as saying:

"The effect of increasing affluence is to minimize the importance of economic goals.

Production and productivity become less and less important" (p. 146)

The waning of interest in production then, could be interpreted as reflecting the post-war economic context as much as any particular dislike of production for its lack of academic respectability. Opportunistic attacks on the value of production teaching might then be taken as just that, opportunism that took advantage of a research problem that was no longer a research problem. The application of quantitative approaches to problems, which Simon (1969) also supports, reflected a broader shift in the social sciences - both as a route to academic respectability, but also, in should be said, to impact on policy. The report of the National Academy of Sciences (1969): Technology: Processes of Assessment and Choice Observed:

"The future of technology holds great promise for mankind if greater thought and effort were devoted to its development. If society persists on its present course, the future holds great peril, whether from the uncontrolled effects of technology itself or for one of unreasoned political reaction against all technological innovation" (p.118).

The National Academy of Sciences saw good data and analysis as the key to interacting with and shaping policy thinking. Their concern was, without intervention, technology production

would escape policy control.

This began to change, of course, with growing anxieties in western economies about competition and productivity. Productivity, since the 1980s, slowly began to be perceived as a problem worthy of attention (and indeed research investment) – but re-framed as an innovation problem. This is a concern that continues today. Evidencing this has been a growing interest in technology change and innovation from research funders and evidenced recently by the UK government promising to spend £32.4bn on innovation and infrastructure, specifically to help close the UK's productivity gap with leading European nations. Certainly, by the 1990s, interventionist industrial policy was firmly back on the agenda in the US, reflecting growing concerns about declining real wages, national prosperity and the state of technology in high-end engineering (Phillips, 1992). In the UK, two key government commissioned reports on construction, the Latham (1994); Egan (1998) Reports, focused on low-levels of innovation and the management styles that caused them. A 2017 McKinsey Global Institute Report recently talked about the need in construction for a 'productivity revolution', citing it as one of the least digitized sectors. Moreover, throughout the 1980s research institutions, linking economics, sociology and business schools, emerged to capitalize on this growing policy interest in, and indeed research funding for, innovation studies. In the UK, SPRU and PREST being prime examples (PREST has now been absorbed by the Manchester Business School). Innovation, industrial growth and productivity issues all also found growing interest from schools of the built environment, engineering, geography and ultimately, business schools.

Koskela rightly points to some of the key practical responses to concern with productivity – Lean, Just-in-Time and Partnering – as coming from industry and not academia. However, this does not mean that academics do not research, reflect and write on these phenomena. For example, the seminal ‘The Machine That Changed the World’ by Womack et al. (1990) on Lean. Koskela also overlooks that some of the most enduring and influential management theories, such as Resource-Based Theory (Grant, 1991; Prahalad and Hammel, 1990; Teece, Pisano and Shuen 1997) stem from business school theory concerned with the innovative capacity of firms, i.e. production. In research into projects and construction management, numerous authors are also concerned with production issues through the study of innovation – drawing on Resource Based Theory, Actor-Network theory, Institutional Theory, Activity Theory, Structuration Theory, theories of materiality (Koskela touches on this) and theories of knowledge. Production also remains a theme that cuts-across many taught courses in the form of innovation and the management of innovation; as an aspect of strategic management, for example.

However, production’s presence in teaching, as Koskela notes, remains limited. Both operations, and to a lesser degree project management, remain a strong aspect of taught programs outside of business schools, at least at Master’s level. Nevertheless, their appearance as mainstream programs within business schools is limited. Arguably, however, this is a problem of demand, not supply and lack of demand reflects the perception, I suspect, that production and innovation remain risky specialisms for students to invest hefty course fees in. The ‘innovation and productivity problem’, in so much as it has attracted growing research and policy attention, has not translated into a resurgence in production jobs. Programming and design are important, but risky propositions given the ease with

which they can be exported. Chemical engineering, for example, has all but disappeared in the UK and construction everywhere is hugely competitive and internationally mobile.

The future of and relevance of production research

Gordon-Howell's desire to see business schools become respectable research institutions has been achieved in many respects. However, it never achieved the objective of creating a consistent body of theory – problem or theory driven. In some respects, this 'failure' is of their own making. One effect of the Gordon-Howell report was to pull business school's away from being dominated by ex-managers drawing on their own experience and opening them up to a broader base of thinking from the Humanities and Social Sciences. As business school's demand for staff grew, this further opened the doors to an increasingly diverse mix of academics - qualitative as well as quantitative; researchers trained in sociology, ethnography, economics (heterodox, evolutionary and neo-classical) engineering, accounting, operations and industrial geography. In the process, this eclecticism has managed to resist, rather than 'failed to achieve', the creation of something resembling a knowledge paradigm.

As a consequence, management researchers are for the most part critical, thoughtful and humble. They recognize that theories are fragile and contingent. This should be regarded as a positive. Not least because it is a style of thinking many try to pass on to their students. Being a diverse community, they are also effective in a variety of different spaces (theoretical and practice-orientated) and, as consequence, are often also effective at

translating theory into practice and indeed back again. Writing about theory, as presented in top journals, may be impenetrable to most managers (and many management academics) but that does not mean that it cannot be translated into useful and applicable knowledge resources for managers and consultant-academics. Quantitative research can debunk taken for granted assumptions about what drives performance, practice research can provide new insights into the changing experiences of managers and this work can inform theory building.

Ultimately, so long as academe remains dominated by the scientific model and the theory-driven research paradigms, then management research will always look and feel like a poor relation. That science has set the measure of prestige should not concern us. Management, as an area of research cannot and should not, seek to develop a consistent research paradigm on which to build an expanding Popperian body of management knowledge. The object of research is not suited to it and such a mission would reduce the eclecticism, debate and innovation that management research presently enjoys.

The future for production research is also brighter than Koskela suggests. In the midst of an economic slow-down and concern over international competition, the conditions are in place for a continued policy focus on innovative capacity and so, by implication, a focus on production.

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